DATE: November 23, 1976

TO : File

FROM : Joshua Lederberg

SUBJECT: Review of Dubois's book just received: "The Professor, the Institute, and DNA", Rockefeller University Press, 1976

This book was first brought to my attention by Abe Eisenstark a few months ago. The press responded with a mail audit inquiry when I wrote them about it, and the book just arrived on my order.

I found the book much more benign about Avery than I think Dubois is in his PRS biography. It is particularly strong in its sensitivity to the constructive aspects of the atmosphere at Rockefeller and the importance of its traditions and devices for interdisciplinary communication. He especially stresses that Rockefeller did not follow the German pattern of an institute built around a single man. Instead (page 33) the institute was a commonwealth of scholars. In fact, I believe that there is still no departmental organization at the Rockefeller University.

Page 44. He stresses the special role of pure chemistry, but the framework in which interdisciplinary thinking was encouraged. He thinks Jacques Loeb was rather narrow minded in his mechanistic conception of life but does view the DNA story as an indication of it. The M.D.-Ph.D. tension is mentioned at several places but, for example, page 46.

Page 47. Many scientists of the earlier part of this century were the sons of Protestant clergymen. (I wonder, for somewhat later period, about the emphasis on the sons of Jewish clergymen). Anyhow, this was a strong element in Avery's biography. However, it appears generally rather superficial and does not have enough knowledge of his private life to be able to comment very meaningfully on such points as his bachelorhood. Something rather strange must have happened to this man between his days at college when he was a great public debater and his pathological reticence in later life. In fact, page 66, one does know about his thyroid problems, 1933 (or 1934?). He was even more withdrawn after that. In his other biography Dubois made a great deal about his thyroid illness having taken him out of the laboratory during the critical time that Griffith's work was investigated. Is there something not quite honest about blurring that over in the present version? Page 69. Dubois seems to take graphology quite seriously! Page 71. "He made little effort to keep up with the details of other fields of science, let alone with other intellectual disciplines". "I was often surprised and at times almost shocked by the fact that his range of scientific information was not as broad as could have been assumed from his fame and from the variety and magnitude of his scientific achievements. Furthermore, his imagination did not seem to me of the kind that soars far above the concrete facts revealed by straightforward observation or by simple laboratory experiments". But then there is later some apology for this. This does not seem quite consistent with what he says later on about, page 152, Avery's efforts to read about kinetics. This does seem to be an explicit anti-erudition.
Page 79. "He had no taste for concepts that did not lead to experimentation".

Chapter 10 is on bacterial variability. This is quite brief and is certainly surpassed by "The Bacterial Cell". There is a little more detail about the acceptance of Griffith in Avery's laboratory.

Page 134. Dubois discusses how Griffith might have discovered transformation and imputes it to an accident. I do not think he has read Griffith's paper very carefully. I think that Griffith already knew that immunity played a very significant role in the selection of variance, and I think it is rather clear that he used mice that had been inoculated with killed smooth cells as a way of establishing a particular immune state, the antibody in which he believed would induce resistance.

Page 135. He criticizes Griffith for saying that the killed S cells functioned "as a pabulum" and claims that "what happens in reality is that the gene corresponding to type 2 replaces the gene...." In fact, again a careful reading of Griffith shows that he very clearly understood the difference between the phenotype and the fact that there needed to be an underlying hereditary factor even if he did not use the contemporary language for it.

It is notable that Dubois does not quote Olby - at least I did not notice it and the name is not in the index - and in fact the introduction, page 3, is quite explicit that he had " consulted only a few of these primary documents and have derived most of my information from semi-official secondary sources and from persons....." He could also have made it clearer that there are essentially no letters or correspondence surviving of Avery unless perhaps he, Dubois, himself has retained copies of them.

Page 136. He asserts that Topley and Wilson 1933 "made only hesitant mention of Griffith". I should check on that.

Page 145. "Prick your own bubble": this is really anxiety about exposure and the possibility of being found wrong.

149-150. Scientific puritanism. "Generally when something as important as this is found, there is a concentration of effort to the exclusion of other avenues of research". This is preemption.

Chapter 12 is a personal analysis of Avery and in many respects this is the most disappointing of all.

Page 152. There is a note that Hotchkiss has retained the collection of Avery's notes on the genetic interpretations of transformation. Dubois himself really passes by the essence of the discovery; its intellectual. He suggests that perhaps MacCarty will write that. I hope so.

This is a puzzling book. I suspect that it is gone through several drafts and that there is an earlier manuscript that would be much more interesting to read but that Dubois decided not to publish.

I should have injected before: page 155. Avery does take Stent to task for his implication that the Avery paper was "premature" and shows many counter-examples to the little impact on genetics that Stent implies. I am a little surprised he does not include my own work on E. coli recombination in the list - work that he does refer to later on in his discussion. Perhaps he simply has overlooked the very brief comments that I have published on the genesis of

that work. I thought I had sent him a reprint of that brief letter to Wyatt, but I am not too surprised that that gets lost in the shuffle in the work of this particular kind. The book is built around his own recollections and history and is no better a job of scholarship than many of the works he has written since 1945 - in stark contrast to "The Bacterial Cell".

On the other hand, the book is still a very valuable document in the light of Dubois's personal relationship to Avery. He says nothing whatsoever of his own history, his disappointments about the development of antibiotics and so forth. It would not have to have been there but I guess it is no surprise that Waxman is not listed in the index either.